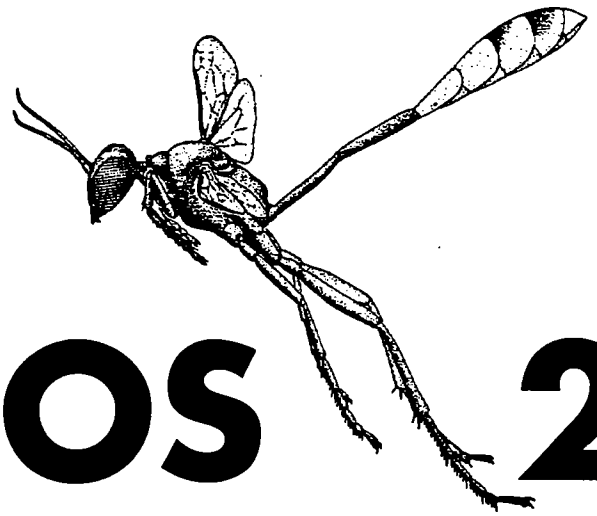

JULY 1995

SPHECOS 29

A FORUM FOR ACULEATE WASP RESEARCHERS

☞ OUR FUNDS ARE ☜ DEPLETED

DONATIONS GLADLY ACCEPTED!

This issue of **Sphecos** has depleted our reproduction fund. It is now zero. Thus more donations will be needed to keep this newsletter going. Your past support has been wonderful and very gratifying, and I hope that some of you will be able to help out again so that we can continue. Duplication costs for a normal size issue are roughly $650 (700 copies).

The meetings of the International Society of Hymenopterists will be in Davis, California this summer (Aug. 12-17). Nancy and I hope to see many of you there. It should be a great meeting. Lynn Kimsey and her gang are going all out to make this meeting a success.

My retirement plans were announced in **Sphecos** 28. I am searching for a replacement editor so that the newsletter does not die. I will bring this up at the meetings in Davis. It is imperative that someone come forward to take over.

I now have my own e-mail address: mnhen023@sivm.si.edu. You can reach me here for regular correspondence and change of address notices. Submissions to **Sphecos** should still be sent to Terry.

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Vera Machado
Elias Cainadas

RESEARCH NEWS

Dick Bohart (Dept. of Entomology, Univ. California, Davis, CA 95616-8584) reports, "I am working on *Bembecinus* again. The two papers underway: *Entomognathus* and *Bicyrtes* are essentially done."

Walter Borsato (Museo Civico di Storia Naturale (Sez. di Zoologia), Lung. Porta Vittoria, 9, 37129 Verona, Italy) writes: "At the present I am preparing a revision of some australian Eumenidae of the genera *Ischnoceolia* Perkins (Eumenidae Discoeliinae), *Australodynerus* G. Soika and *Stemdyneriellus* G. Soika."

Bolívar Garcete Barrett (Sección Invertebrados, Mus. Nac. Hist. Nat. Paraguay, Sucursal 19 Campus, Ciudad Univ., Central XI, San Lorenzo, Paraguay.) writes, "I am presently working on systematics, biogeography and biology of polistine wasps in Paraguay. In addition, with Massimo Olmi, I'm preparing a list of Dryinidae of Paraguay, and with the help of Jim Carpenter I'm planning to study the long-overlooked collection of A.W. Bertoni. I am also the curator of Hymenoptera at this museum."

Arkady Lelej (Inst. Biology and Pedology, Vladivostok-22, 690022, Russia) "I am now finishing the third paper in the series on Smicromyrmini with a review of six genera (four of them new). I plan to continue my work and study of another difficult group – Oriental Trogaspidiini – and prepare a key to the genera of this tribe. It will take at least one year or more and I'll try to receive a grant (you know about the serious problems in Russia and my salary – less than US$ 100 per month, not even enough for food).

Peter van Ooijan (Prof v. Bemmelenlaan 61, 3571 El Utrecht, Holland) says, "Not much research news, my Pompilid collection has gone to the ITZ Amsterdam, and after reorganising their Dutch collection and halfway reorganising the Palearctic collection I quit pompilids. My Sphecid collection, including the type specimen of *Tachysphex picnic*, has joined the collection of the RMNH at Leiden.

"As for myself I am working on software, rearing tropical fish, trying to rear aculeates in my garden and wondering

in which way I will re-enter the field of biology. It is good to read **Sphecos**, keeps me in shape."

U.K. TELEPHONE NUMBERS CHANGE

Please note that **ALL** U.K. telephone numbers changed on April 1st 1995. An extra 1 goes into the town code. My current town code is 0342; this will become 01342, or, when phoning from overseas, 1342.

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ADDRESS CORRECTIONS

The following are corrections to the mailing list addresses given in the last issue of SPHECOS.

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PEOPLE IN THE NEWS

Dr. Max Fischer, entomology curator at the Naturhistorisches Museum, Vienna, Austria, retired at the end of December, 1994.

OBITUARY

George R. Ferguson
(January 8, 1915- June 24, 1994)

George Ferguson passed away last year at the age of 79. The following account of his life is taken largely from an article that appeared in the September 1986 issue of the college newsletter, *The Oregon Stater*, but we have also had access to his obituary published in the June 26, 1994 issue of the newspaper *Corvallis Gazetter-Times*. – editor.

George was born in Bolivar, Louisiana, the son of Lloyd and Ethel Collins Ferguson, but he grew up in southern California. He attended the University of California, Berkeley but received his bachelor's and master's degrees from Oregon State University. Ferguson left OSU to continue his studies at Ohio State University, where he earned a doctoral degree in entomology in 1941.

"I decided to specialize in the chemical control of insects," he notes, "because that's where the jobs were." Although he returned to the Oregon State U. Agricultural Experiment Station as assistant entomologist in 1941, he was to leave again in 1943. His major professor had recommended him for a research position on a special project at the University of New Hampshire that was financed by Swiss-owned Geigy Corporation. "Being young and adventurous," says Ferguson, "I accepted the offer at the University of New Hampshire – a one year postdoctoral appointment – because it involved a 'secret' compound and promised to be a challenging project."

The secret compound, which had been smuggled out of Switzerland by Geigy Corporation during World War II, turned out to be DDT. That substance would have enormous impact toward the end of the war, when it was still strictly controlled by the military, and after the war, when it was finally released for general use. During the war, DDT was used successfully for lice control to eliminate typhus epidemics in Southern Europe and to control mosquitoes and malaria in the South Pacific. After the war, it proved of great benefit in agriculture.

Ferguson's career and success from the time he moved to the University of New Hampshire were inextricably tied to the development, testing, and production of DDT. He was soon asked to look at the possible uses of DDT in agriculture, and he carried out laboratory and field tests using some of the techniques that he had developed for his doctoral research. At the end of World War II, when many chemical firms went into the production of DDT, Ferguson recommended to Geigy Corporation that it should set up its own laboratory to develop DDT and other products. He then became chief entomologist and technical director of the Agricultural Chemicals Division, Geigy Chemical Corporation, a position he held until 1953. At that time he was asked to regroup that division, and he became president. "It was my job to lead the company out of the red by developing new products," says Ferguson. "We opened two additional large plants – in Alabama and Louisiana – and produced other chemicals. I had the satisfaction of turning a business around and seeing the company grow from five to 150 million dollars a year."

Ferguson became executive vice-president of Geigy in 1969 and vice-president of CIBA-Geigy in 1970 following the merger of the two firms. He retired from the corporation in 1972.

After retiring from his productive career in the corporate world, George moved back to Corvallis, Oregon in 1973 from Scarsdale, New York, so that he could resume the systematic study of wasps at his old alma mater. He was given a courtesy appointment in the Department of Entomology at Oregon State University where he assisted graduate students. George dedicated his time to studying wasps, and organizing and classifying a portion of the large insect collection at OSU. He donated his substantial worldwide collection of wasps (more than 80,000 specimens) to OSU.

George first became interested in bees and wasps in the thirties when he took a course on beekeeping at Oregon State University from entomology professor Herman Scullen. While his professional career was in the field of insect control, his keen curiosity about wasps became a life-long avocation that took up most of his spare time. In retirement George pursued studies of sphecid wasps in the genera *Cerceris*, *Eucerceris* and *Philanthus*, possibly influenced by his early contact with Herman Scullen, who worked on these wasps for many years. Apparently George hoped to publish revisions of these genera, and although his knowledge of them was vast, the revisions unfortunately never saw the light of day. However, between 1981 and 1984 he published nine papers on these genera in which he described new species, keyed species of certain species groups, clarified the status of many names, established lectotypes, etc.

Ferguson's Wasp papers

1976. The distribution and origins of northwest sphecid wasps. Bull. Oregon Ent. Soc. (61):492.

1981. Synonymy and distribution records in the genus *Eucerceris* (Hymenoptera: Philanthidae). J. NY Ent. Soc. 89:172-183.

1982. Descriptions, synonymy and sex associations in the genus *Eucerceris* (Hymenoptera: Philanthidae). J. NY Ent. Soc. 90:147-160.

1983a. Two new species in the genus *Philanthus* and a key to the *politus* group (Hymenoptera: Philanthidae). Pan-Pac. Ent. 59:55-63.

1983b. The types of cercerine wasps described by Nathan Banks (Hymenoptera: Philanthidae). J. NY Ent. Soc. 91:223-234.

1983c. Descriptions of two previously misidentified species of North American *Cerceris* and related synonymy (Hymenoptera: Philanthidae). J. NY Ent. Soc. 91:235-241.

1984a. (with Colin Vardy). *Vespa semipes* F., a junior synonym of *Cerceris arenaria* (L.) (Hym., Philanthidae). Ent. Monthly Mag. 120:55-57.

1984b. Revision of the *Philanthus zebratus* group (Hymenoptera: Philanthidae). J. NY Ent. Soc. 91:289-303.

1984c. The types of some American *Cerceris* with lectotype designations (Hymenoptera: Philanthidae). J. NY Ent. Soc. 91:431-441.

1984d. An annotated synonymic list of North American and Caribbean wasps of the genus *Cerceris* (Hymenoptera: Philanthidae). J. NY Ent. Soc. 91: 466-502.

Reminiscences of Dr. Kunio Iwata
by
Karl V. Krombein

My first contact with Dr. Kunio Iwata was in 1950 when he suggested that we exchange copies of our publications. Iwata had already established himself as a pioneer in the comparative approach to wasp behavior with his germinal work, *Comparative studies on the habits of solitary wasps* (1942). His interest in wasps traces back to the publication of the Japanese translation of Fabre's *Souvenirs Entomologiques* (1924).

A letter from him in 1951 indicated that he had made substantial progress on his monumental studies on Hymenoptera ovaries (1955–1966), having already dissected about 500 species. Somewhat later our correspondence dealt with the work each of us was doing on trap-nesting for solitary wasps and bees. Iwata was using bamboo sticks, and called the occupants tube-renters, When he acknowledged receipt of my trap-nesting book in 1967, he wrote that there were more than 50 species nesting at his home in Sasayama.

We finally met during my first trip to Japan after my visit with Katsuji Tsuneki (**Sphecos** 27: 6–7). Dr. Iwata met me at the Osaka airport just after I retrieved my luggage. During our trip to the hotel, he told me that our mutual friend, Keizo Yasumatsu, mentioned that I enjoyed conviviality. So, when we reached my room, I got out the bourbon, we had several drinks and talked about our families and wasps. He said that the next time I came to Japan, I must stay at his small house in Kobe. Kunio then gave me his volume in Japanese, *Evolution of Instinct: Comparative Ethology of Hymenoptera*, inscribing it, "This is the first copy. Feb. 13, 1972." He had just received the volumes from his publisher. I urged him to consider translating the book and publishing it in English.

The next morning he took me on a trip to Kyoto, the ancient capitol of Japan. We visited the Imperial Palace where the Emperor's coronation occurs, Nijo Castle, former residence of the shogun, and many temples, pavilions and gardens. We had a delicious Japanese luncheon at the 400 year old Kitoh Restaurant. It caters to the emperor when he visits Kyoto. The cook wanted to make a special dish for me, her idea of what an American would like; it turned out to be a poached egg on top of a rice cake with slices of pickled pumpkin. Our waitress was impressed with my white hair, asked Iwata how old I was, and volunteered that she was 41 and a spinster. We continued sightseeing until mid–afternoon, returned to Osaka, and said farewell after a final toast. That evening I wrote in my journal, "... I wish that we'd met in person long ago."

When I returned to Washington, I wrote Iwata to thank him for his generous hospitality, and urged him to prepare an English translation of his *Evolution of Instinct*. He declined, writing modestly that it was "... only a tedious summation of already published data." Iwata agreed though, that I might try to have it translated in the Smithsonian's program. It was included in the NSF program using PL 480 funds available in India for the translation and publication. I also obtained a grant in PL 480 funds for Kunio to travel to Delhi to edit the translation.

During September 1973 I was able to accept the Iwatas' kind invitation to stay at their home in Karato, Kobe, for several days. Kunio met me at the Osaka railroad station following my visit with Tsuneki. We took an electric car to Kobe, and then taxied through Mt. Rokko to Karato. Mrs. Kazuko Iwata welcomed me, but I did not meet their two sons who were away in college. She spoke English fluently, and was an accomplished teacher. After tea and several beers, I had a refreshing soak in their hot tub, and enjoyed a bountiful dinner prepared by Kazuko. After dining, Kunio and I shared some Suntory scotch, and discussed plans to travel together when it was time for him to check the final translation.

After breakfast the next day Kazuko prepared sandwiches for three and a large thermos of tea. We journeyed by trains for several hours so they could show me the large, impressive Himeji Castle. Kunio and I talked about hymenopterists and wasps during the lengthy train trips. He told me that in Japan people who enjoyed conviviality were called tigers. We agreed that from then on he would be known among ourselves as Black Tiger, our mutual friend Yasumatsu as Yellow Tiger, and I as White Tiger.

On my last day Kunio had arranged for a friend at Kobe University to drive us to the university. There I had a chance to see their insect collection including the specimens collected in Thailand by Iwata, and to meet fellow hymenopterists, Tikahiko Naito (sawflies), Setsuya Momoi (Ichneumonidae), and T. Okutani. Momoi then drove us to view the tranquil Sorakuen Garden in the middle of Kobe. After lunch at the Rokko Club, we went to the top of Mt. Rokko, and later walked through the Kobe Municipal Arboretum. We had a brief visit to meet Mrs. Momoi, have some refreshments, and then Momoi drove us back to Karato. After dinner the three of us taxied to Kobe, and thence by train to Osaka. Kunio and I had a last bourbon in my hotel room, and we then walked back to the railroad station to say our farewells.

The Indian publisher wrote me in mid-December 1973 that the translation would be ready by mid-March 1974. Kunio and I made plans for our travel, he got his passport and visas, and began getting the necessary series of inoculations. In mid-February the publisher wrote that the translator had fallen seriously ill, so we cancelled our travel plans. Subsequently, we decided to check the translation by air as increments were forwarded by the publisher. The book was finally published in July 1976.

Kunio did not attend the International Congress of Entomology in Kyoto, 1980, but he made a special trip on 6 August to spend a few hours with me. We had hoped to have a reunion of the three Tigers, but Yasumatsu was busy with the International Committee, so he missed the beer and sake drinking. The two of us exchanged bottles of whiskey, American bourbon and Japanese scotch. We also wrote joint postcards to several absentee wasp behaviorists. After an early dinner we walked to the railroad station for his return home.

He wrote in April 1982 that he had undergone a gastrotomy in January because of stomach cancer, and could no longer drink beer or whiskey. Kunio reported that in good weather he was "... gardening or observing insect lives." He continued until 1987 to set out bamboo sections at his home to attract twig–nesting wasps and bees.

Most of Iwata's important contributions in entomological journals are listed in the bibliographies of wasp and bee literature in his book, *Evolution of Instinct*, 1976. However, he was a pro-

lific writer on insect natural history in his native language. He published a series of volumes, 1943-1983, that he called his own "souvenirs entomologiques". He translated the titles as *Memoranda of a naturalist* and *Fifty years observing insect life.* The series consisted of vignettes of behavior of a variety of insects but concentrated on solitary wasps and bees. More than half of the 150 titles were never published in entomological journals.

He loved children, and published several books just for them. At the elementary level he designed a kindergarten book on *Polistes* in 1971, entitled *Ashinagabachi* (long-legged wasps); the text and illustrations were by H. Kubota and N. Tomioka respectively. In 1974 he published a book for older children, *Lives of wasps and bees*, with photographs by H. Oda. This book was awarded the prestigious Mainichi Publications Culture Award for 1974 from the Mainichi Newspapers.

In 1982 he published an elegant book, *Japanese Wasp and Bee Life Illustrated Phylogenetically.* The text is by Iwata, and the 84 color plates of numerous, excellent photographs of adults and nests are by his co-authors, K. Kozima, M. Matsuura and K. Goukon. In my letter acknowledging receipt of this handsome book, I congratulated Kunio on the splendid contribution that would enable the layman to appreciate the beauty and complexity of the animals that we love so much. I commented on the quality of the photographs that were so sharp, with color so true, and with a wonderful depth of focus. Regrettably, the book is out of print; there are no plans to republish it.

I am grateful to Kazuko Iwata for furnishing the following biographical data for Kunio. He was born 25 May 1906 in Osaka. The family moved in 1910 to a residential suburb, Ikeda. His father died in 1917, leaving his widow to raise Kunio and five sisters in needy circumstances.

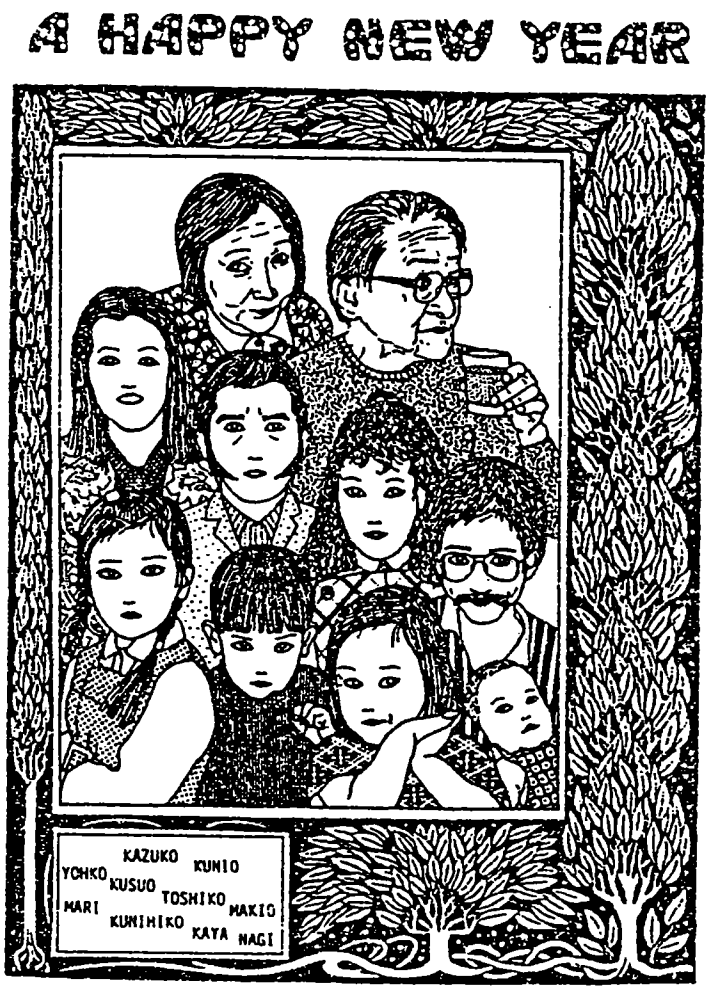
Iwata received his Master's degree from the Agricultural Department of Kyoto University in 1931, and remained for several years as an unpaid assistant in the laboratory. Between 1934 and 1941 he taught biology in several high schools. He submitted his D.Sc. thesis to Kyoto University before his departure to Hainan Island, China, and subsequently was awarded the degree. Kunio was a research member of Kihara Biological Institute, 1942 to 1946. He was repatriated after the war, but all of his data and records of those years were lost.

When Iwata returned to Japan he taught first at Kagawa Agricultural College, and then at Hyogo University of Agricultural. The latter institution later became the Agricultural Department of Kobe University. In 1961 he was in Thailand for five months searching for natural enemies of agricultural pests, a period during which he continued his behavioral studies of wasps and bees. Kunio retired from Kobe University in 1970 because of the age limit.

He married Kazuko Toshima in 1948. They had two sons, Kusuo (1949) and Makio (1953), and there are five grandchildren, a boy and four girls.

Kunio died 29 November 1994, and a memorial service was held on 10 December at Takigawa-Kinen Kaikan of Kobe University.

It seems appropriate to conclude these reminiscences with a copy of Iwata's personally drawn New Year's card for 1988 showing him in a characteristic pose with his family. He was a genial, modest man, a cherished friend, and a superb scientist.



Kunio Iwata
Kanato dai. 2-18-3
Kita. Kobe. JAPAN

HELP NEEDED

The Museo Nacional de Historia Natural del Paraguay is an institution dedicated to investigating the diversity of life in Paraguay. Although I am only a volunteer of the museum at the moment, I am actually the curator of the Hymenoptera. My current work is to make a computerized database of the collection; identify specimens to the highest level that I can and at the level our small library allows; keep contact with specialists on Hymenoptera and send material for identification; organize and keep up the collection, etc. Therefore, I request that anyone interested in seeing hymenopteran material from Paraguay (and also from adjacent countries) belonging to any group, and could identify it for me, and also anyone who could contribute literature, to please write to me.

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Distribution of West Palaearctic *Ceramius* species (Vespidae, Masarinae)

by

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In the summer of 1994 I found the honey wasp *Ceramius tuberculifer* de Saussure at two localities in southern France (Dept.: Alpes Hautes Provence). This encouraged me to start working on the distribution of the species. *Ceramius tuberculifer* has been confused with *C. lusitanicus* Klug, at least until the revision from Richards (1962), and is also close to *C. bischoffi* and *C. vechti* (both described by Richards 1963). Therefore Richards in 1963 stated that the distribution of *C. tuberculifer* "now needs further study". As far as I can tell, this has not been done. For that reason I would like to study as many specimens of west palaearctic *Ceramius* as possible. Please contact me if you have such material in your collection. I would like to borrow the specimens for determination. I am also interested in field observations, such as habitats, flower visits, nest aggregations, etc.

References

Richards, O.W. (1962): A revisional study of the masarid wasps (Hymenoptera, Vespoidea). London.

Richards, O.W. (1963): New species of *Ceramius* Latreille (Hymenoptera, Vespoidea) allied to *Ceramius lusitanicus* Klug. Zoologische Mededelingen 38: 213-220.

FORUM

On names, taxa, and categories - some remarks on phylogenetic systematics and nomenclature

by

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Germany

In 1994 I finished an analysis of the phylogenetic relationships within the Sphecinae using morphological characters and based on the theory of phylogenetic systematics in the strict sense of Hennig. Unfortunately, some major North American hymenopterists appear to be quite irritated due to the practical consequences of the strict use of the theory of phylogenetic systematics for an analysis on a relatively low categorial level. As one of these hymenopterists was chosen to review the manuscript, the first attempt to publish the results in an international German journal was unsuccessful as he recommended that the editors not to accept the manuscript. It must be emphasized that neither the reviewer nor anybody else have ever criticized any part of my morphological analysis or of my results. What was criticized was the way these results have been translated into a cladogram and how the resulting monophyletic taxa have been named. Similarly different opinions about the acceptance of phylogenetic systematics appear to exist among the students of other insect groups as well. Unfortunately, some of the problems a few hymenopterists have with this concept of representing phylogenetic relationships are the result of misunderstandings of important terms and principles I have used in accordance with Hennig, Ax, Griffiths, De Queiroz, Gauthier, and others. Hence, this statement attempts to explain the concept of naming within the frame of phylogenetic systematics as I have used it in my analysis of the genera of the Sphecinae and as it will be used in further phylogenetic analyses in the future. I hope that my statement will initiate a dialogue in **Sphecos** on what was recently called 'phylogenetic nomenclature' or 'phylogenetic taxonomy' (De Queiroz 1992, De Queiroz & Gauthier 1990, 1992, 1994) and its significance in hymenopteran nomenclature.

To make my point clear, it is useful to repeat the definitions of some important terms that will be used in the following text. The definitions have been compiled from different sources (Ax 1987, Mayr & Ashlock 1991, Simpson 1990, De Queiroz 1992, De Queiroz & Gauthier 1990, 1992, 1994):

(1) A **taxon** is a named group of individuals, independent of whether or not this group is a monophylum.

(2) The **names** of the taxa must be regarded as proper names (e.g. Apidae, Hymenoptera, *Sphex*).

(3) "A **category** designates rank or level in a hierarchic classification.... Terms such as species, genus, family, and order designate categories" (Mayr & Ashlock 1991: 21). It must be emphasized that a category is an abstract term that must be distinguished from the name of a taxon. While a category must be regarded as a class name, the name of a taxon designates real entities in nature if it is a monophyletic group or a biospecies.

(4) The terms **systematization** and **classification** must be carefully distinguished: systematization refers to the practice of hypothesizing phylogenetic relationships and to recognizing monophyla. Hence, the goal of a systematization is the reconstruction of relationships existing in nature, that of a classification is subjective definition. The term "classification" refers to the erecting of artificial classes of elements on the basis of freely chosen specific characteristics.

Let us choose the subfamily Sphecinae as an example. Numerous biopopulations taken together form about 800

species which, depending on their phylogenetic history, are members of different **taxa**. These taxa are **named**, e.g., *Sceliphron*, Sphecini and Prionychina. All together they form the actual **taxon** whose proper **name** is Sphecinae. In the traditional classification each named taxon like Sphecinae has been assigned a categorical rank to indicate its position in the categorical hierarchy. Thus, the term "subfamily" is a **category** designating the rank of the taxon with the proper name Sphecinae in the traditional classification.

As De Queiroz & Gauthier (1994: 27) have shown, "the current nomenclatural system is clearly non-evolutionary." The most accepted method that accomplishes this goal is provided by the theory of phylogenetic systematics sensu Hennig (I prefer to use the term 'phylogenetic systematics' rather than 'cladistics' to emphasize the methodological differences. For detailed explanations of the theoretical basis of phylogenetic systematics see e.g. Wiley 1981 and Ax 1987). Given that the central principle of phylogenetic systematics is the recognition and characterization of species and monophyletic taxa one may ask what role a system of biological nomenclature can play in this scientific process. As the existence and the recognizability of natural entities are independend of the way scientists name them, the naming of taxa has no influence on any scientific process that is part of a phylogenetic analysis. Only following the recognition and characterization of the natural entities one should ask which name is the best for each entity. As scientists need to communicate with each other it is necessary to give each of these entities a proper name. The function of a taxon name is to refer unambiguously to a certain taxon. The set of rules and principles that govern the selection and the use of taxon names to exclude or at least minimize ambiguity, that is synonymy and homonymy, is called a nomenclatural system.

In summary, species and monophyletic taxa can be and have to recognized in nature. Any relationships between the scientific process of analysing phylogenetic relationships and the naming of recognized taxa do not exist: the structure and the type of taxon names need not reflect any information about the taxa they name to gain unambiguity but are only a question of conventions.

Nevertheless, ever since Linné's nomenclatural system based on the assignment of the so-called Linnaean categories like "familia" and "ordo" started the development of the present rules of the ICZN any taxon name has to be connected to such a category to adhere to the rules. One can say that categories lead to a classification of given taxa, that is, taxa are ordered into subjective classes (Griffiths 1976). As has been shown above the assignement of a proper name to a certain taxon is principally sufficient to gain unambiguity in a nomenclatural system. What information content does the additional category express to justify its existence and that could not be expressed by the proper name itself? Is there any logical reason why named monophyletic taxa must be classified? The subjectivity of using categories is clearly seen in the often discussed problem concerning the "best" categorial rank to assign to the main bee subgroups. Some hymenopterists prefer to say "subfamily Colletinae", while nowadays the majority advocates to use a "higher" rank, that is "family Colletidae" (e.g. Michener 1986, Michener et al. 1994). Unfortunately, there is no logical and scientific reason for how one could come to prefer one possibility rather than the other. The same is true for the "Sphecidae". While the vast majority agrees with the use of subfamilies in Bohart & Menke (1976), Albert Finnamore (in: Goulet & Huber 1993) "elevates" the sphecid subfamilies to family level. Menke & Pulawski (1993) wonder about Finnamore's argument for doing so, that is to "... make the classification comparable to that widely accepted in the Apiformes" (Finnamore), is never reversed: "Why not make bees comparable to sphecids and recognize only Apidae" (Menke & Pulawski). Indeed, their question is justified but one cannot expect a satisfying answer to it (that is a scientific one). The conflict clearly shows the arbitrariness in assigning categories. The widely accepted usage of subfamilies in Bohart & Menke is simply a result of convention induced by the comprehensiveness of their monumental study. Nevertheless, as Finnamore's family category as well as Bohart & Menke's subfamily category lack any scientific foundation or even requirement, it is impossible to find any scientific reason to prefer one of them. (Nevertheless, one may ask if the names of the sphecid subgroups in the form **Larrinae** rather than **Larridae**, independent on whether they are traditionally ranked as a subfamily or a family, should be protected as well-established names in the -inae form used by Bohart & Menke to gain stability.) Anyway, the point is that the discussion about the "best" category does not lead to a better understanding of the groups studied. Furthermore, this discussion appears as if it deals with a scientific problem while it is just one of formalism.

It should be stressed that many more difficulties appear when one attempts to adapt the classification system of the Linnaean categories to a phylogenetic system. The encaptic hierarchy of sister groups with their identical rank leads to the demand for identical categories in a classification system. Due to the high number of sister groups the use of categories is very limited. For example, this is easily seen in Byron Alexander's (1992) comprehensive analysis of the subgroups within the Apoidea that are traditionally ranked as tribes. If we recognize, in accordance with common practice, the Apoidea as a superfamily and one of the most basic groups like Laphyragogini as a tribe, innumerable additional categories are necessary to classify each pair of sister groups between these categorial ranks. Farris (1976) has attempted to solve this problem proposing eight prefixes to increase the number of possible categories. Nevertheless, verbal constructions like "Gigapicotribe" even more show the subjectivity of assigning categories to taxa: Who would be able to decide between "Gigapicotribe" and "Megapicotribe" depending on what a scientist believes to be the best for his purpose?

This problem is closely related to the term "stability" with respect to nomenclature. Stability in the sense of the ICZN aims at the uniqueness and distinctness of the taxon name itself (in combination with the category assigned to this name). As this is correct, taxon names are able to change their meaning, that is a taxon name is related to slightly different taxa in different times, depending on the scientific progress. This situation leads to confusion and to the requirement to supplement old names with more information to specify what a certain scientist means. E.g. using the name Vespidae makes it necessary to specify if one refers to the monophylum including the Euparagiinae, Masarinae, Eumeni-

nae, Stenogastrinae, Polistinae, and Vespinae (Carpenter 1981), or to the monophylum consisting of the Stenogastrinae, Polistinae, and Vespinae only (Richards 1962). If, however, stabilisation concentrates on the relationship between a taxon name and the named taxon rather than the name itself it is possible to avoid these difficulties. This leads to exactness and unambiguity in nomenclature. E.g., the phylogenetic analysis of the Sphecinae mentioned above the "Sceliphrini" (sensu Bohart & Menke 1976) is regarded as a paraphyletic group as I found evidence for a sister group relationship between *Stangeella* cyaniventris[1] and (Sphecini + Ammophilini). The remaining "Sceliphrini" without *S. cyaniventris* have already been named Sceliphrina by Bohart & Menke. Thus, there is an unambiguous relationship between the taxon names and the named taxon: "Sceliphrini" = Sceliphrina + *S. cyaniventris*, Sceliphrina = "Sceliphrini" without *S. cyaniventris*. As the "Sceliphrini" are regarded as a paraphyletic, hence artificial grouping this taxon has to be eliminated from the phylogenetic system. The (proper) name of the taxon consisting of the "Sceliphrini" without *S. cyaniventris* is Sceliphrina with regard to priority and, thus, should be used to name the sister group of (*S. cyaniventris* + (Sphecini + Ammophilini)). As a result, the use of Sceliphrina and the rejection of "Sceliphrini" gains maximal nomenclatural stability: These taxon names refer to the same taxa in both Bohart & Menke and the phylogenetic analysis. Nevertheless, I was asked by the editors of the journal mentioned above to redefine the meaning of "Sceliphrini" with respect to the loss of *S. cyaniventris*. The result would be a case of synonymy ("Sceliphrini" = Sceliphrina) and a change in what is named by "Sceliphrini": from including *S. cyaniventris* to excluding it. In this case the name "Sceliphrini" would be still existing, but the taxon it should name would be difficult to assess.

In recent publications it has repeatedly been shown that it is possible to present comprehensive phylogenetic analysis exactly and unambiguously without Linnaean categories (e.g. Griffiths 1976, Ax 1987, De Queiroz 1992, De Queiroz & Gauthier 1990, 1992, 1994). To stabilize the relationship between the proper name of a taxon and the named taxon it is important to gain this unambiguity. However, the Linnaean categories are still widely accepted among scientists, but more and more of them are becoming sceptical. I am well aware that a concept of a nomenclature based on phylogenetic principles is less than ready today. Nevertheless, however unfamiliar the abandoning of Linnaean categories may be, it is based on a well founded theoretical concept.

[1]=Subtribe Stangeellina of Bohart & Menke, p. 87 - editor

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NEW NEWSLETTER

Socium

The Russian Language Section of the International Union for the Study of Social Insects (IUSSI) is producing a new newsletter, *Socium*, edited by Vladilen Kipyatkov. Issue 2 has both Russian and English articles, and includes short communications, news and views, announcements, and a bibliography.

Comprised of specialists from Russia, Belarus, Ukraine, Armenia, Moldova, and Turkmenistan, the Russian Language Section was formally accepted into the IUSSI on August 27, 1994. The Section intends to organize colloquia every two years and publish the resultant proceedings as a periodical.

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HYMENOPTERA DATABASE

I have created on the PC a program in DBase IV for listing the species of Hymenoptera. The program includes 13 groups of Hymenoptera (Table A) and it is possible to expand it. For each genus there are 4 database files (Table B): the first for species, the second for subspecies, the third for synonymies of species and the fourth for synonymies of subspecies. Every file is used for printouts and searches (Table C).

At present the number of species included in the program is:

Group	Species	Subspecies	Syn. spec.	Syn. subspe.
A	1067	94	1285	22
B	660	25	283	3
C	6342	75	2839	0
D	1687	36	563	3
E	1061	305	340	23
F	330	95	82	4
G	2245	559	1219	106
H	2630	663	1424	163
I	1440	268	891	115
L	3033	229	1551	51
M	3736	467	1141	89
N	2338	249	1271	88
O	6914	895	2932	180
Total	33483	3960	15821	847

Every species record is composed of:
- Name of the genus
- Valid name of the species
- Author
- Year of publication
- Name of nominate genus
- Corology
- Further notes.

Some of these fields may be lacking. The listed species are from all over the world but with a preference for the Holarctic Region. In the list there may be some mistakes; it is a basis for subsequent work.

If there are entomologists interested in having a copy of my program and database, they may send me a diskette

E L E N C O S P E C I E H Y M E N O P T E R A

SCELTA DEL TAXON DA ELABORARE

Symphyta		A – Tutte le famiglie
Apocrita	Terebrantia	B – Ichneumonoidea+Evanioidea
"	"	C – Chalcidoidea
"	"	D – Proctotrup.+Cynipoidea+Altre
"	Aculeata	E – Chrysidoidea
"	"	M – Scolioidea
"	"	F – Formicoidea
"	"	G – Vespoidea+Pompiloidea
"	"	O – Sphecoidea
"	"	H – Colletidae+Halictidae+Melittidae+Apidae
"	"	I – Andrenidae
"	"	N – Megachilidae
"	"	L – Anthophoridae

Scegli il taxon da elaborare (anche per terminare)

Table A

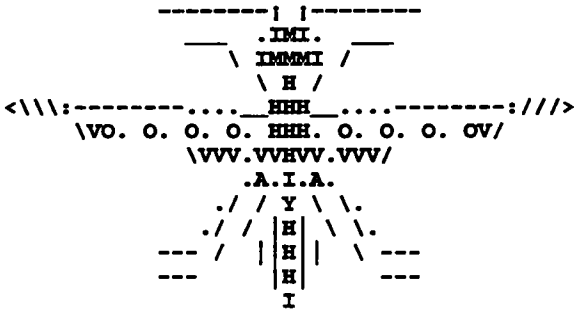
S T A M P A

A – Checklist specie italian
B – Elenco genere attuale
C – Elenco generico italiano
D – Elenco specie Collezione
E – Situazione spec. singola
F – Elenco per gruppo specie
G – Ricerca nome nel Genere
H – Ricerca nome nell'Elenco
I – Elenco genere nominale
L – Elenco alfabetico Generi
M – Elenco nomi per Autore

U – Fine della elaborazione

Operare scelta prego >> <<

Table C



	Specie	Subspecie	Sin. specie	Sin. subspecie
NQ in Archivio	6914	895	2932	180
NQ in Elaboraz.	4	1	1	1
Dim. file Arch.	1099326	142305	419276	29340
Dim. file Elab.	636	0	143	0

TAXON scelto: SPHECOIDEA Genere in elaborazione: Rhinocorynura

(I) Inserire — (M) Modificare — (A) Annullare — (G) Aggiungere — (S) Stampare — (T) Togliere annullati
(N) Scelta nuovo taxon — (C) Copiare archivio — (E) Ripristinare archivio — (Z) Finire

Table B

(3.5", 1.44M) for every taxon (maximum 13 diskettes, program included). In exchange I wish to have specimens of Mutillidae from any region. Entomologists are free to decide the quantity of Mutillidae to send me.

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SCIENTIFIC NOTES

Ageniella evansi, a cavernicolous spider wasp

by

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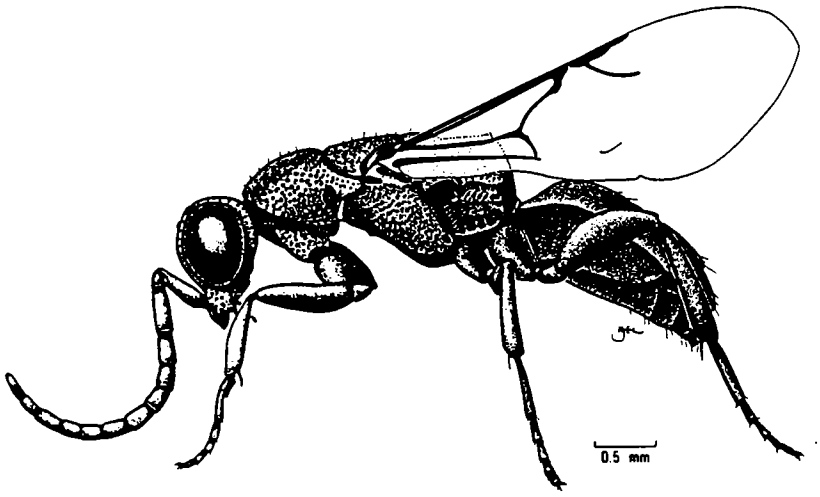
Species of spider wasps (Pompilidae) illustrate some of the most interesting nesting habitats and intriguing behavioral patterns among the aculeate Hymenoptera. Unique and unusual behaviors or the results thereof can be seen in the aquatic and shoreline adaptations of *Anoplius depressipes* Banks, *A. eous* Yasumatsu and *A. ithaca* (Banks)(Evans 1948, 1949, Evans and Yoshimoto 1962, Ricards 1969, Roble 1985, Shimizu 1992), host web nesting site of *Allochares azureus* (Cresson) (Deyrup et al. 1988), and host spider's silken prey cachement of *Calicurgus hyalinatus* (Fabricius)(Kurczewski and Spofford 1985). Atypical pompilid nesting habitats have recently been described for *Ageniella fulgifrons* (Cresson) – inside of an abandoned mole tunnel (Kurczewski and Kurczewski 1987), *Auplopus caerulescens subcorticalis* (Walsh) – within holes and cracks in a concrete basement foundation (Kurczewski 1989a), and *Tachypompilus ferrugineus* (Say) – between the base and upright portion of a cemetery monument (Kurczewski 1989b, 1990). However, nothing has been more amazing than to learn of the nesting habitat of *Ageniella evansi* Townes – deep within the confines of a darkened, underground cavern!

On February 18, 1992 I received a hand-written letter from Robert Pape, an experienced spelunker from Tucson, Arizona. Mr. Pape had observed a small, metallic-colored wasp inhabiting Arkenstone Cave in Colossal Cave County Park, Pima County, Arizona. The cavern is located in the upper part of the Sonoran desert scrub biome near the foothills of the Rincon Mountains. Mr. Pape described the wasp provisioning with "delimbed" spiders, transporting them deep into the cave "to the edge of total darkness, occasionally going deeper by following our headlights." Mr. Pape had tentatively indentified the wasp as belonging to the genus *Auplopus* and, because of its small size, metallic coloration and behavior of amputating all of the spider's legs, the species obviously belonged to the pompilid tribe Auplopodini (Evans and Yoshimoto 1962) [actually the correct tribal name is Ageniellini Banks – see Day, 1979, 1981, 1988 -editor]. Mr. Pape asked me to confirm the identification of this pompilid and he subsequently sent me the prey spider and a male and female wasp. The species proved to be *Ageniella evansi* about which no behavioral or ecological information is known, except for two prey records and montane habitat (Townes 1957, Evans 1959). The spider was identified by Vince Roth as belonging to the genus *Selenops* in the rare family Selenopidae. There are no records of selenopid spiders comprising pompilid prey (Shimizu 1994). I returned the specimens to Mr. Pape and, under separate cover, forwarded to him all of the information I could find on Nearctic species of *Ageniella*, including reprints and xeroxes of papers from my own files and the Cornell University entomological library, excerpts from Hymenoptera catalogs, etc. I proposed a plan of study for him to use, if interested, that would incorporate making important behavioral and ecological observations because what he had seen and described to me represented potentially new and significant scientific findings. Some of the discoveries that Mr. Pape later made and reiterated to me were remarkable, especially when one considers that he had to lie down, hugging the cave floor, squeeze through narrow cavities, and ascend and descend unlit subterranean cliffs in order to follow, observe and photograph the wasp.

After receiving three lengthy and informative letters from Mr. Pape, the last being dated June 20, 1992, I lost total communication with him. Periodically, for nearly three years, I have tried to reach him by letter to no avail. My attempts to reach him by telephone in the Tucson area have invariably ended with, "Mr. Robert Pape does not have a telephone listing." I guess this qualifies him for **Sphecos'** list of missing persons! In my last letter to him, before losing communication, I had recommended that he publish all of this information somewhere because the observations he had been making were detailed and important. I indicated that, if he would gather the data, I would help him put together a manuscript to



Myrmecomimesis nigrithorax (Riek), male
(Chrysididae, New South Wales, Australia)

submit for publication. In an attempt to find out whether or not Mr. Pape had published these observations, I searched through various biological, entomological and natural history journals from the southwestern U. S. After striking out I telephoned Howard Evans, Karl Krombein and Arnold Menke, all avid aculeate Hymenoptera literature perusers, but none of these individuals had seen anything in print about a cave-inhabiting pompilid wasp. I have been advised to repeat Mr. Pape's interesting and valuable observations in this journal before I misplace or forget them.

Mr. Pape's notes on this species extend from October 7, 1990 to May 17, 1992. Both males and females of *A. evansi* were active during the months of March, April, May, September, October, November and December. In addition there are two specimens of this species in the University of Arizona Insect Museum, both from high elevations in mountains, collected in July and October. The sum of these collection and observation dates indicates that *A. evansi* is probably multivoltine in the region. When Mr. Pape revisited Arkenstone Cave in June, July and August 1991 he saw no wasps, suggesting a moderately lengthy period of summer diapause at this locality. Ambient (air) temperatures outside of the cave during periods of observation averaged 29°C and inside of the cave, 21°C. Relative humidity in the cavity remains a constant 100%.

Observations of wasps with and without prey were made rather continuously from late morning (1019-1100 hours) to mid-late afternoon (1430-1630 hours) on certain days. A total of 17 wasps exited the cave from 1019 to 1152 on April 4, 1992. Before exiting, females paused, cleaned their antennae, wing–flicked, remained motionless for 30 seconds or so and walked or flew away. The first female with prey entered the cave at 1206 of that day. Hunting forays occurred outside of the cave, mostly between the hours of 1000 and 1200. Provisioning and nesting activities took place within the cavern mostly after these hours. Wasps with prey spiders penetrated the cave into total darkness to distances of 30-60 meters from the cave entrance in order to reach their nesting sites. Except for one flight, all provisioning wasps walked or ran on the cave floor, "seldom" on the walls. Speed of transport depended upon the differential sizes of the wasps and their prey, the directness of the route taken and the number of obstacles encountered. One provisioning female took eight minutes to walk and run 23 meters. During prey transport, the wasp "placed her mouthparts near the tip of the abdomen of the spider just above the apex (dorsal). She then approached the spider from the right rear and grasped it in the (normal) fashion by a chelicera (dorsally) and proceeded to haul it off. Several times she stopped to rub her hind legs together or run her antennae through cleaners, never once releasing the prey." In a photograph showing prey transport, the wasp's long, thin antennae and hindlegs are angled forward and backward, respectively, possibly to obtain tactile information about the unlit immediate environment. At times, two wasps traversed the cave floor "side by side" or in tandem. Mr. Pape believed that this behavior was "not...totally random." As many as four wasps were simultaneously seen in one "room" of the cave. Some wasps became agitated when near other females. Retrieval of one wasp's abandoned spider by another female was observed.

Provisioning wasps quickly entered "small holes" in the walls of the cave, disappearing entirely from sight. One female entered the same hole twice with successive prey and as many as two or three wasps disappeared into a single hole. Because the walls of the cave mainly consisted of solid rock no provisioning cells could be located. Twenty–seven spiders taken from provisioning wasps were all identified as *Selenops* sp. (see above). Color photographs sent to me by Mr. Pape show that the spider's legs had been amputated at the coxal-trochanteral joints and sometimes the pedipalps had been partly or entirely removed, often unevenly so. The prey spiders were not "free–living" inside of the cave; rather, they lived outside of the cave entrance where the wasps hunted for them in "thick vegetation." Only a "few" males were encountered inside of the entrance to the cave and never deep in the cavern. A single male-female *A. evansi* interaction involved a four second-long antennal "exchange" (= touching?) on the floor of the entrance.

In Mr. Pape's last letter to me, he asked the following questions: (1) Why do the wasps wing-flick only outside, not inside of the cave?; (2) Why do they never relinquish their grasp of the prey during transport?; (3) How do the wasps navigate within the dark confines of the cave? Do they follow a chemical trail, air movements within the cave or floor landmarks?; (4) How do they pre-select their nesting sites?; (5) What are the nests (cells) like and how are the immature stages protected from parasitism and predation?; (6) Will females accept artificial nesting tunnels? In this correspondence Mr. Pape included diagrams of (A) routes of two provisioning *A. evansi;* (B) random exit paths of 12 females at cave entrance; and, (C) artificial nesting chamber design (with dimensions).

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Nesting behavior of *Trypoxylon (Trypargilum) rogenhoferi* Kohl (Hymenoptera, Sphecidae) in a flooded "varzea" forest of central Amazonia

by

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The nesting behavior of *Trypoxylon rogenhoferi* was studied in a Central Amazonian "varzea" flooded forest. Nests were gathered in trap-nests made of wood drilled longitudinally to a depth of 80 mm with apertures of 9.5 mm and 12.7 mm diameter respectively.

The nest architecture and most of the behavioral characteristics of *T. rogenhoferi* were largely similar to those observed in other species of the genus. The nests consists of a linear sequence of prey stocks separated by mud partitions. Nesting activity was greater in the 9.5 mm diameter traps than in those of 12.7 mm aperture. Observation of the behavior of *T. rogenhoferi* at the immature stage provide answers to questions posed by some authors, such as the use of mud in the construction of the cocoon wall. The larvae of *T. rogenhoferi* take bits of mud from the internal wall and mix them with the silk mesh to form a girdle round the body. The mud from the girdle is subsequently spread throughout the cocoon wall. Another question is the function of the mud deposited by the female at the inner of the cavity, the preliminary plug. It was observed that the larvae produce a fragile-walled coccon if a source of mud is not available. The preliminary plug in nests of *Trypargilum* species serves as a source of mud for the larva of the first cell.

The cells were only provisioned with *Alpaida veniliae* (Keyserling)(Araneidae). The male, in contrast to the females, developed in the innermost cells and were more abundant in the nests of 9.5mm diameter. The hatching rate from the cells provisioned by *T. rogenhoferi* was 41%, mortality from unknown causes 38% and mortality due to parasitoids 21%. The parasitoids most frequently involved in the destruction of cells were *Pleurochrysis postica* (Brullé), *P. morosa*, *Neochrysis lecointei* Ducke and two unidentified species of *Caenochrysis* (Hymenoptera, Chrysididae)(60.7%), followed by *Lepidophora* and *Anthrax* (Diptera, Bombyliidae)(23.3%). Nesting activity was greater in the falling water phase which coincides with the period of least precipitation. It is suggested that the seasonality of *T. rogenhoferi* is dependent on prey availability, which in turn is influenced by the flood cycle.

Sleeping Aggregation of Neotropical Sphecinae

by

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An interesting example of a sleeping aggregation was observed in Tafí del Valle, c. 2,500m (on label; probably nearer 2,000m) in the Argentine province of Tucumán in early January 1970.

Some time after mid-day heavy cloud began to cover the sky and some spots of rain fell. As I was walking along, looking for anything worth investigating, my attention was attracted by some red spots in two closely-adjacent bushes (?*Baccharis* sp.; Asteraceae). These spots turned out to be the basal parts of *Ammophila* gasters (species as yet undetermined). Among them were also a few specimens of *Prionyx bifoveolatus* Taschenberg; the wasps were all males. Each wasp had the mandibles clamped around a vertical stem, the legs holding it lower down, and the body held approximately horizontally (perpendicular to the stem).

A Daytime Resting Aggregation of Male *Pepsis sericans* (Pompilidae)

by

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St Augustine, Trinidad
and
Luis R. Hernández L.
Habana, Cuba

On 27 June 1994 at about 15:00 we came upon a loose aggregation of spider wasps in farmland near Maisí, Guantánamo province, Cuba. It was a hot, sunny day toward the end of the dry season in an exceptionally arid part of the island. The wasps were immobile, occupying two branch ends about 1.5 m from the ground on an *Acacia farnesiana* tree. Resting aggregations of solitary wasps and bees are more commonly observed at night than during the main part of the day.

We succeeded in netting a sample of 13 wasps, amounting to about half of the aggregation. Examination showed that all were male *Pepsis sericans* (=*domingensis*) Lepeletier. One individual in the sample corresponded unequivocally to the description (Alayo 1954) of *P. domingensis ignicornis* Cresson, while the others were all of the typical colour form. The specimens are deposited in the collections of the University of the West Indies and the British Museum (Natural History).

Alayo (1954) noted that, while males of *P. domingensis ignicornis* are commonly collected, the female is unknown, and remarked that "I suspect that this is a case of an exclusively male variety." The finding of the two discrete forms together goes against their recognition as subspecies and supports Alayo's implied suggestion of a sex-linked dichromism. The alternative explanation – that the two are separate species –

seems much less likely in view of the persistent failure to find genitalic or other structural difference.

We thank Colin Vardy for advice on *Pepsis* taxonomy.

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Apparent Bird Predation on *Trypoxylon* Brood

by
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The following observations are from Abraham "Bram" Willink's argentine country house at Tafí del Valle, Tucumán (1985m), mid-December 1993. On the outside walls of the house and outbuilding I found numerous disused mud nests of an unidentified *Trypoxylon* sp., each with up to about 30 cells. The cells lay parallel to the wall, forming a narrow comb up to three cells broad, i.e. no cell was separated from the wall by more than two cells. I saw no adult wasps or nesting activity at that time, early summer. A nest of apparently the same species in the Instituto Miguel Lillo collection is associated with wasps identified as *T. fabricator*. A quick look at it suggests that it is indeed a member of the *fabricator*-group, but the wasp seems too small and the nest unlike that of *T. fabricator*.

A peculiar feature of most nests was considerable, fairly systematic damage, such that cells were opened along most of their length, exposing the cells interiors. It did not have the appearance of haphazard damage from weather or house-cleaning. Suspecting that birds had opened the cells in search of wasp brood, I looked for nests in relatively bird-protected situations and found some behind window gratings. These were not completely enclosing, so that a small, agile bird could be expected to reach the nests, but it would have required some maneuvering and would have placed the bird in a situation from which it could not quickly escape.

Censusing on the two buildings, I found the following ratios of damaged:undamaged nests:

exposed surfaces 44:2
behind gratings 3:11.

The result seems plain enough, but anyone should feel free to run a chi-square test.

Bram tells me that he has often seen wrens about the buildings, although he has not noted them attacking *Trypoxylon* nests. The house wren, *Troglodytes aedon*, would seem to be the best candidate.

I am unaware of other observations suggesting that birds systematically open mud nests of any solitary wasp.

Daytime Censuses as an Estimator of Colony Size in Small-colony Wasps

by
Christopher K. Starr
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From the researcher's point of view, an important virtue of most small-colony wasps (stenogastrines and independent-founding polistines) is the ease with which the entire colony can be observed in/on its open nest comb. It is customary to census colonies at night, under the reasonable assumption that then and only then are all adults likely to be present. However, nighttime censuses are not always practical, which raises the question of the reliability of daytime censuses. Are there circumstances in which counts taken during daylight hours can serve as acceptable estimators of the true number of adults in the colony?

Despite the popular view of social-insect colonies as scenes of intense activity, with foragers leaving and returning at a great rate and much of the workforce away from the nest at any moment, experienced bug-watchers have long noticed that even during the active period, much of the colony much of the time is doing nothing in particular (e.g. Wheeler 1957). This tendency is quite pronounced in small-colony wasps, so that it is probably fair to say that at any given moment most adults are probably at home.

As an example, in order to collect complete colonies of *Polistes olivaceus*, *P. stigma* and *Ropalidia marginata* during daytime in the Mariana Islands, Miyano (1994) first collected all wasps present at each nest and then waited at least one hour to net any returning adults, on the reasonable assumption that a forager was unlikely to be away for more than an hour. From 14 founding-stage (i.e. before emergence of the first workers) and growth-stage (i.e. with workers present but no reproductive offspring yet emerged), each with a maximum of 13 adults, he collected a total of 45 adults initially and 16 that returned later. In other words, only about 1/4 of adult females were absent from the nest at once. Furthermore, almost half of the colonies had no wasps returning during the waiting period, so that the entire colony was probably present in the initial collection.

This raises the possibility that the highest figure from a series of daytime censuses of a colony could be treated as an acceptable estimate of the true number of adults resident on the nest. How many censuses should it take to reach such an estimate? My purpose here is to report a very small data-set from one species, which nonetheless seems quite suggestive.

During 3-4 July 1994 in the Dominican Republic's Parque Nacional del Este, I did seven daytime and three nighttime censuses of each of 11 founding-stage colonies of *Polistes crinitus* on nests with up to 27 cells. All brood was quite young, apparently consisting of eggs and 1st-3rd instar larvae.

A surprising result is the inconstancy between nighttime censuses of a single colony. Although the numbers did not vary greatly, in only three of the 11 colonies were all three censuses identical, even though no colony had more than six adults.

Only one colony (with a single adult) was constant across all seven daytime censuses. However, even here the variation between one census and the next was not very great. In fact, if one looks only at the first three and the last three daytime censuses, the numbers of colonies constant for all the three censuses are four and three, respectively, virtually the same as at night. Nonetheless, the average number of wasps present at night is somewhat higher than in the active period, as expected.

If numbers are inconstant even at night, should the lowest or the highest number be taken as reflecting the true number of resident adults? I will evade this question by noting that I am not concerned here with what it means to be "resident" on a nest but with the degree of similarity between daytime and

nighttime censuses. It makes sense, then, to treat the modal nighttime census of a given colony as the one against which daytime censuses will be compared. In 9/11 colonies the mode was at least as great as any of the other eight censuses for the colony. Taking the highest nighttime figure, rather than the mode, would alter the comparison a little, but it would be inconsistent with the standard assumption that one nighttime census is enough to completely census a colony.

A particular daytime census is scored as "correct" if it equals or exceeds the nighttime mode. For the 11 colonies, the number of correct daytime censuses varied from one to seven out of seven. Overall, 48/77 (63%) of daytime censuses were correct. Given the simplest statistical assumptions, this means that in three daytime censuses the probability that at least one correct census is just about 95%. Taking once again the 22 sets of the first three and the last three daytime censuses from the present data-set, we find that 20 (91%) each had at least one correct census.

The early stage of all colonies in this data-set is certainly significant. The time of these observations was toward the end of an extended dry season, when there was very little forage for the wasps and colony growth seemed to be very slow. A month later, with more demanding, late-stage larvae present, the adult females would presumably spend a greater part of the active period away from the nest. It may be that already in the growth stage there is usually so much activity that some wasps are off the nest throughout the active period, in which case daytime censuses may not work at all. However, it is my experience that the active period in small-colony wasps does not occupy all of the daylight hours.

I do not want to make too much of this small and very limited data-set. The figure of three daytime censuses cannot very well be general for small-colony wasps or even tropical *Polistes*. Still, the data make a prima-facie case that under certain conditions just a small number of daytime censuses can yield a reliable estimate of colony size.

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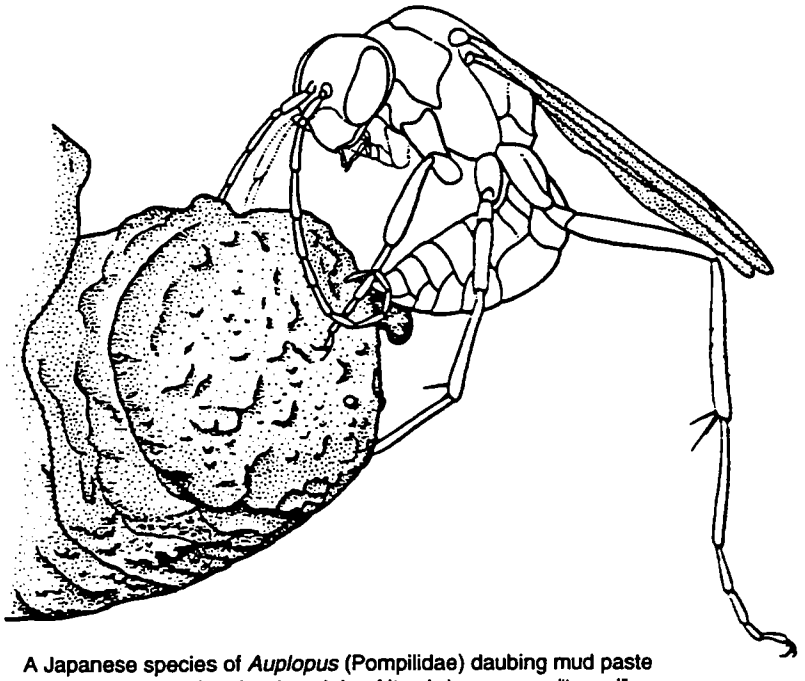

A Japanese species of *Auplopus* (Pompilidae) daubing mud paste on its nest by using the dorsal tip of its abdomen as a "trowel".

MUSEUM/COLLECTION NEWS

More on Tsuneki Type Material

In **Sphecos** 28:24-27 Nuhn and Menke listed all of the Tsuneki holotypes in the Collection of the National Museum of Natural History, Washington D.C. At the end of that compilation, we listed 39 non-Japanese Tsuneki species whose types were not in our collection. We assumed that the holotypes for these taxa were returned to the collectors by Tsuneki. However, Tadashi Tano of Fukui City, Japan, informed me in a letter that all of the missing types were found in Tsuneki's Collection. Mrs. Tsuneki apparently has agreed that they should be sent to the Smithsonian to join the rest of the non-Japanese holotypes of Katsuji Tsuneki. Tano (1994a, b – see Recent Literature) has just published a list of the Japanese type material of Tsuneki that is held in his collection.

Current plans are to place the Tsuneki Collection, including his Japanese holotypes, in The Museum of Nature and Human Activities, 6 Yayoigaoka, Sanda, Hyogo 669-13, Japan.

Arnold Menke

The Types of V.S.L. Pate at The Academy of Natural Sciences of Philadelphia

by

Harry W. Allen (deceased)

[The following manuscript was found among materials of Harry Allen at the Smithsonian - editor.]

Unfortunately, the location of the type specimens of a number of Hymenoptera species described by the late V.S.L. Pate was not indicated in the original descriptions. All the types known to be present in the collection at the Academy of Natural Sciences have been checked. Those found there are listed below. In preparing this list the author acknowledges the very helpful assistance of Dr. Karl V. Krombein of Washington D.C. The following abbreviations have been used: [H] for holotype, [A] for allotype.

No type labelled *Motes muspa* was found. However, a specimen bearing the following information on its labels

was found. "Miakha St. Pk., Fla., Feb. 18, 1937, ♂; Type Motes miakha." Since this information is identical with that accredited to the type, the specimen is presumed to be the type of *muspa*.

The species of Sphecidae in which Pate stated that the type was deposited in the Academy collection, but have not been found there, are as follows: *Ectemnius (Hypocrabro) alpheus*, *E. (Hypocrabro) texanus ais*, *E. (Hypocrabro) satan*, *Psammaecius (Hoplisoides) alaya*.

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AMERICAN INDIAN EXPLANATION FOR HOW THE YELLOWJACKET GOT HIS STING

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Vaa káan pakuntáxraatvanaatihanik koovura pakaan kuntáxraatvanaatihanik Úytaahkoo.
Long ago they were chipping obisidian there, they were all making their obsidian arrowheads there at Mount Shasta.
Koovúra pakêemishas pakóo ára píipvanaatihansan, vaa kumakêemishas.
They were all the monstrous and mean animals who would be stinging people in the future, those kinds of vicious beings.
Vaa uumkun koovura pakaan kuntaxraatvanaatihanik.
They were all making obsidian arrowheads there at Mount Shasta long ago.
Vaa kunipítihanik, "Vaa mûuk Yías'ára nuykáratiheesh, pananúsaak mûuk."
They were telling each other how they would be killing human beings with their arrowheads (in the future).
Ápsuun pa'ishimfiréeshiiphanik músaak.
Rattlesnake's obsidian point was the deadliest.
Mahxánthuun káru pa'ishímfir pamúsaak.
Scorpion's arrowhead was also deadly.
Víri vaa uum káru pishpíshi u'ifiktihanik pásaak pataxrátraam.
Yellowjacket was there, too, picking up little obsidian flakes at the arrowhead workplace.
Vúra pufáathara tûupichas u'ifiktihanik pásaak.
The arrowheads he was picking up were little obsidian flakes amounting to nothing.
Kóomahich vúra poomfírahiti pishpíshi.
So, nowadays yellowjacket's arrowheads hurt for just a little while.

Adapted from "Pishpíshi," in Ararapikva: Creation Stories of the People, *edited and translated by Julian Lang, published by Heyday Books in 1994.*

NEW BOOKS

Nomenclator Zoologicus, vol. VIII, 1966-1977. Edited by Marcia A. Edwards and Mary A. Tobias. Published by The Zoological Society of London, Regent's Park, London, NW1 4RY, England. 620 pages. Published 1993.

It has been nearly 20 years since the last volume appeared in this important resource on generic names. The current book covers the taxa published from 1966 to 1977, and contains approximately 33,000 generic names. The editors apparently plan on additional volumes which they say should be completed "without undue delay" because they will have access to *Zoological Record* tapes. Included in the present volume is an addenda for the first seven volumes, and corrections to volumes 1-7. The addenda also includes names received too late for inclusion in the main section of volume 8.

Arnold Menke

The Dryinidae and Embolemidae of Fennoscandia and Denmark by Massimo Olmi. 1994. Fauna Entomologica Scandanavica 30:1-100. ISBN 90-04-70224-8

This book is a detailed overview of the biology, and systematics of the Dryinidae and Embolemidae from the northern European region of Denmark and Fennoscandia. It is organized in three basic components. The first begins with a thorough examination of general biological aspects of each family, including morphology, biology, parasites, economic importance, fossils and evolution. This introduction is followed by a detailed discussion of the taxonomy and classification taxa in each family that occurs in this region, with keys to genera and species. For each species there is a description, geographic distribution, and specific information on hosts and bionomics. Finally, the last part of the book consists of a large table of host records, and 38 pages of beautiful watercolors plus color photographs of these insects.

The only real flaws in this work involve a number of basic assumptions made by the author in the phylogenetic discussions. It is clear that sex associa-

tions are very difficult, particularly in the Dryinidae, and that males are far more conservative structurally. This fact probably makes males more valuable for generic analyses than females. However, the author bases his phylogenetic statements on females. Females are so highly specialized for parasitic behavior that deriving a phylogeny for the Dryinidae based on female characteristics may show little useful resolution. Additionally, it makes no sense to do a phylogenetic analysis of species found in such a small, biogeographically unremarkable region. The species found here undoubtedly have sister species in other, not necessarily adjacent regions, not just in the area of Denmark and Fennoscandia. The cladogram on page 32 is a classic of its kind.

However, overall this book is a thorough, and valuable work. It is clearly and concisely written, and contains one of the most detailed treatments of the biology and morphology of these families ever published. It certainly contains the most beautiful illustrations of these wasps I've ever seen.

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ALLOTYPES

For some years now, I have been serving as a review editor. This has exposed me to the writings of authors from all over the world. I have been amazed by the fact that some taxonomists have no real appreciation of the term allotype. For example, some writers will describe and designate the allotype of a previously described species! Such action is simply improper. Why? The explanation is simple. When an author describes a new species, a holotype is designated, and any other type specimens cited in the original description are paratypes. Some authors designate one paratype as an "allotype" to indicate that it is the opposite sex of the holotype. But that does not change the fact that it is still a paratype. If an author describes a new species from only one sex, then anyone that subsequently finds the previously unknown opposite sex can describe it. But it is improper to identify one specimen as the "allotype". Type material (holotype and paratypes (and allotype) can only be designated in the original description. Subsequent descriptions of an unknown sex are simply that.

Arnold Menke

DERIVATION OF SCIENTIFIC NAMES

Providing the derivation of the names of new species is something that authors occasionally omit in their original descriptions. The consequences of this can sometimes be disconcerting, and in the case of patronyms, downright dishonorable. Occasionally a species name is published with a spelling different from that intended by the author. This happens because an author may miss a typesetting error during reading of proofs, or he or she may, in some cases, not even see proofs. Under the provisions of the Code of the International Commission on Zoological Nomenclature, specifically Article 32, the original spelling of a name cannot be emended unless there is clear evidence in the original description of the intended spelling. I offer two examples that illustrate this point.

I described a new species of *Ammophila* from Utah (Menke 1966) and called it *uinta*, after the Uinta Indians of that region. The printer spelled the name *unita* throughout the description, and I did not see the error during proof reading. Unfortunately, I did not give the derivation of the name, so there was no evidence in the original description itself that would permit me to emend the name to *uinta*, my intended spelling. Thus the species will forever be *unita*.

The North American hymenopterist, S. A. Rohwer, described (1910) a new species of *Pemphredon* (he actually used the generic name *Ceratophorus*), and the published spelling was *gennelli*. Rohwer did not state in his description that he was dedicating the species to the American lepidopterist, Fordyce Grinnell, Jr., although the specimen on which the description was based was collected by him and apparently "grinnelli" was the intended spelling. The Code is very clear here (see Art. 32(c)(ii) and examples). Unless there is explicit evidence in the original description of the intended spelling, the name must stand as printed. Since Rohwer did not say that he was naming the species after Grinnell, the species must forever be called *gennelli*.

These two examples clearly demonstrate the desirability of providing the derivation of any new species name. This is especially true for species named after people. If you think highly enough of someone to name a species after them, you should tell the world that you are naming the species in honor of "John" or "Jane Doe". Otherwise the honor is lost, and you have no recourse if somehow the name is misspelled when published.

Arnold Menke

Menke, A. S., 1966. New species of North American *Ammophila*, Part II (Hymenoptera, Sphecidae). Proc. Biol. Soc. Wash. 79:25-40.

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ABE MAKES COMMERCIAL NEWS

[Abstracted by Robin Edwards from an article in The Nikkei Weekly, Tokyo: Vol 32, No.1644, October 31, 1994, page 5.]

A Nikkei staff writer reports on Takeshi Abe's creation of a sports drink containing some of the compounds found in the saliva of hornet larvae. Abe, of the Institute of Physical and Clinical Research in Japan, figured that this saliva must be responsible for the amazing power that enables adult hornets to beat their wings more than a thousand times a minute, and to fly over 100km a day. His analysis of the saliva showed it contained large quantities of the amino acids, glycine and proline.

Abe's concoction has been tried by marathon runners, rugby and ice hockey players, and all have reported improved results if they drink the "potion" before play begins. The actual contents of the drink are not reported!

[Abe and his colleagues, principally Kawai and Niwa, have published numerous papers on the composition and properties of hornet venoms – see my Bibliography. R.E.]

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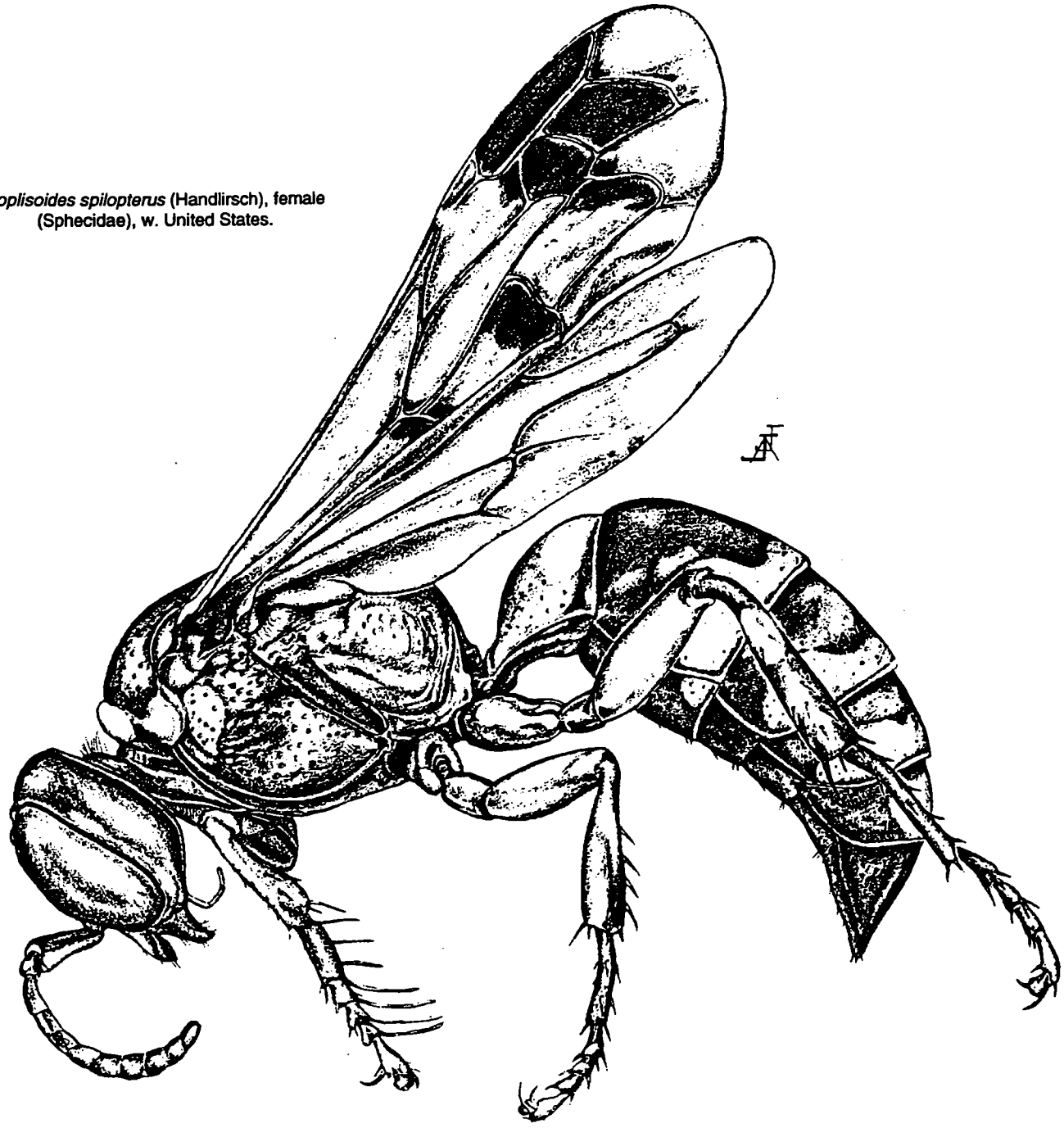
YO, ARNOLD, THIS ONE'S FOR YOU

Brian Freeman and I were wandering the Hellshire Hills, a wild xeric area near Kingston, Jamaica. Watching the wasps flying near our feet, Brian espied a busy *Larra* (Sphecidae). "You see", he quipped in reference to superstar Brian Lara of the West Indies cricket team, "Lara is not just a cricketer." When I complimented him on his tremendous pun, Brian look puzzled, thinking my praise overdone. He had forgotten that *Larra* hunts only crickets. Sometimes you just sort of stumble on these things.

Chris Starr

[a funny pun, Chris, for *Larra* is unrecorded from Jamaica. Possibly Brian saw a *Liris*, or a new distribution record for *Larra* – editor]



Hoplisoides spilopterus (Handlirsch), female (Sphecidae), w. United States.

IVth INTERNATIONAL COLLOQUIUM ON SOCIAL INSECTS

First Announcement

The *Russian Language Section of the International Union for the Study of Social Insects* announces its IVth International Colloquium which will be held in St. Petersburg (Russia) from Friday 16 till Thursday 22 August 1996. The Colloquium will cover all aspects of behaviour, ecology and physiology of social and presocial arthropods and will be international with a broad participation of colleagues from other IUSSI sections and other scientists from abroad.

The official languages of the Colloquium will be English and Russian. The scientific meetings are scheduled for four full days, the other two days being devoted to excursions all over St. Petersburg, its beautiful palaces, museums and environs. An additional excursion tour could be organized for two days after the Colloquium closure.

Papers presented to Colloquium (up to 25 typewritten pages in English or in Russian) will be published in the IV volume of the *Proceedings of the Colloquia on Social Insects*. The authors will receive 50 reprints of each article without charge.

We would be happy to see all our foreign colleagues among the participants of the IVth International Colloquium on Social Insects in St. Petersburg.

If you intend to participate, please, send us a short application (see below). We will send out the second announcement at the beginning of 1996.

Communication:

Dr. Vladilen E. Kipyatkov, President of the IUSSI Russian Language Section,
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Preliminary title of a talk/poster.....

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Visit Hong Kong

Free accommodation in Hong Kong for professional entomologists interested in visiting Hong Kong for up to two weeks for collection or studies of local oriental insects.

For further information please contact Dr. Mike Crosland, Biology Department, Chinese University of Hong Kong, Shatin, N.T., Hong Kong.

In the last year we have had entomologists from 5 different countries visiting and collecting. We plan to further increase the number of visiting entomologists in the coming year.

HAWAIIAN TERRESTRIAL ARTHROPOD CHECKLIST

Second Edition — Gordon M. Nishida, Editor

Newly Revised, this checklist includes corrections and additions to the first edition. The checklist covers Hawaiian insects, mites, spiders, and other terrestrial arthropods, and lists all valid names, residency status (e.g. endemic, adventive, purposefully introduced), and distribution by island. Included in the 8,552 total are 5,264 endemic, 2,621 adventive, and 416 purposely introduced species. The index in this version has cross references to older names no longer in use (due to synonymies, misidentifications, etc.).

The Checklist is extracted from a comprehensive information-base on Hawaiian insects and relatives being developed and maintained by the Hawaii Biological Survey at Bishop Museum.

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ENTOMOLOGICAL BIBLIOGRAPHY OF THE CALIFORNIA ISLANDS

At long last, the California Islands Entomological Bibliography is available on the Internet. It is on the Biodiversity Gopher server (muse.bio.cornell.edu 70) under "Biodiversity Resources/Faunas" and will soon be mirrored on the Bishop Museum Gopher (bishop.bishop.hawaii.org 70). I'm still working on the entomological database (some 3500 species) and hope to release it to Gopher eventually.

This bibliography includes 1073 citations on the entomology of the California Islands. Most of these were previously published in Menke and Miller (1981), Miller (1985) and Miller (1993). Taxonomic coverage includes insects, arachnids, and myriapods (i.e., all terrestrial and freshwater arthropods except Crustacea). Geographic coverage includes all the California Islands: the Channel Islands, Los Coronados Islands, and the islands in the San Francisco Bay Region. Islands off Baja California, Mexico, are excluded. Papers contributing to any area of knowledge of California Island entomology are included except for those only giving checklist entries. A few of the most important general references are also included, especially those dealing with the plant communities and geological setting of the islands.

The annotations include reference to the order, family, and species studied (unless there were too many list), and references to plant and vertebrate hosts. The abbreviation "TL" means type locality. Literature search for this bibliography was updated to September 1994. Further supplements are planned as sufficient additions accumulate; additions and corrections should be directed to Scott Miller.

This bibliography was compiled from 1978-1994 using the facilities of the Santa Barbara Museum of Natural History, Natural History Museum of Los Angeles County, Museum of Comparative Zoology, Smithsonian Institution, and Bishop Museum. Many people and institutions are acknowledged in the published bibliographies. F.G. Hochberg, C. L. Hogue, and A.S. Menke were especially important in encouraging the continued development of this bibliography. S. Bunting, C.E. O'Connell, and K. Kami assisted in compiling the present form of the bibliography. This bibliography was improved during projects funded by the National Park Service, Island Research Fund of The Nature Conservancy, and Santa Barbara Museum of Natural History.

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